

pages 412 to 414 of the 15th volume of the *Journal of Geology*, a review signed T. C. C.,<sup>11</sup> in order to obtain that writer's opinion about the marine origin of the terraces.

Dr. R. T. Chamberlin further assumed that the animal remains were swept by floods into the positions they occupied. No proof can be afforded that a single bone was thus carried into those creek deposits, although this transportation would not involve their belonging to a Pleistocene stage older than that of the deposit No. 2. However, the animals found there probably died not far distant.

In his efforts to prove the recency of the mammalian remains and the deposits at Vero, Dr. Chamberlin hit upon two ideas which have come to other minds since that time, if not before, and which appear to have given them much comfort. These are (1) that the southern climate was better adapted for mammalian life than that of the northern States and (2) that the mammalian fauna existed longer there than it did elsewhere. These notions appear to inspire a sort of poetical feeling, for the conditions are spoken of almost always as "that genial southern clime" and the animals are tenderly mentioned as "lingering longer there."

Doubtless during the Wisconsin glacial stage the mammals of the northern regions were forced southward, even into Florida and Texas. Reindeer reached Kentucky, musk-oxen migrated to Oklahoma, *Elephas boreus* (*E. primigenius*, of authors) probably strayed as far south as Florida and Texas, and so with many other northern species. When, however, the glacier retreated these animals did not remain there, but they kept as near the glacial front as they found it comfortable. Mastodons and certain elephants doubtless lived in Florida during the wane of the Wisconsin stage, but there is not a whit of evidence that they lived there at a later time than they did in New York or Michigan. For a reindeer and a musk-ox the genial climate is the one which furnishes plenty of snow and the kind of food they need.

Now as to the matter of lingering, it is a certainty that many of the mammals found in the Pleistocene beds at Vero, Peace Creek, Melbourne and many other places in Florida did linger there and elsewhere and become extinct only at a later time. *Myiodon*, one or more species of tapirs, the great ox *Bison latifrons*, and *Equus complicatus* appear to have lived on until the Sangamon interglacial. The American mastodon, *Elephas columbi*, and the giant beaver lived long after

<sup>11</sup> Journ. Geol. 15: 412-414. 1907.

the disappearance of the Wisconsin glacier and left their bones in the deposits overlying the drift. It is also true that many other species, specifically unchanged, are still lingering and they constitute the existing fauna of North America. With the species named above there existed at Vero, Peace Creek, and Melbourne *Megatherium*, *Chlamytherium*, *Glyptodon*, *Elephas imperator*, the Florida saber tooth tiger, and one or more camels. In regions farther west and northwest, as at Frederick, Oklahoma; Rock Creek, Texas; "Hay Springs" (Peters) Nebraska, and in the Aftonian deposits of western Iowa, there are found also numerous species of horses, camels, *Mastodon mirificus* and *Elephas imperator*; and these appear not to have existed anywhere after the first interglacial stage. Had they continued to exist their remains ought to be found in the deposits overlying the Kansan, the Illinoian, or the Wisconsin drifts. Outside of the drift region, in the Appalachian ranges from Lookout Mountain, Tennessee, to Frankstown, Blair County, Pennsylvania, in caves and fissures, have been collected numerous species of mammals of apparently mid-Pleistocene times, but none of those mentioned as being characteristic of the first interglacial stage. In northwestern Arkansas an abundant fauna has been discovered in a fissure, but among these were no *Elephas imperator*, no *Mastodon mirificus*, no camels, no *Glyptodon*, no *Megatherium*, no *Chlamytherium*. In the Mississippi embayment, extending from Cairo, Illinois, to the Gulf and on the south from western Louisiana to western Alabama, a very interesting fauna has been collected, consisting of mastodons, elephants, one or two species of horses, tapirs, megalonyx, mylodon, etc.; but again the forms which are taken to be peculiar to the first interglacial, or Aftonian, stage are not found. We are justified, I maintain, in believing that, instead of a few lingering here and there some hundreds of thousands of years, perhaps to conduct to the happy hunting grounds the spirit of some "mid-Recent" red man, they ceased existence near the close of the first interglacial, or at most did not live beyond the Kansan glacial. Therefore, I hold that the creek bed No. 2, at Vero, and its contents belong in the first, or Aftonian, stage of the Pleistocene.

During his second visit to Vero Dr. Chamberlin was especially engaged in determining, at the localities where human remains had been discovered, the relations of beds Nos. 2 and 3 at their plane of contact. His purpose was to learn whether the human remains were really found in No. 2 or in what he regarded as the very recent No. 3. As to the skeleton No. 1, the first one found, he thought that the 9

inches of brown sand overlying it was too thin to permit a safe conclusion.

At the locality of the second skeleton, where there had occurred more vigorous stream action, Dr. Chamberlin carried the plane of contact nearer the layer of shells. His conclusion was evidently that the human bones belonged in the muck layer or at least might have belonged there. That he proved this he certainly would not assert; nor would he perhaps regard it as necessary. The writer believes for the reasons stated above that it can not be successfully contested that the stratum No. 2 is of early Pleistocene age. In case the muck layer belongs to the Recent epoch we may inquire what was the condition of that little valley during the intervening 200,000 or 300,000 years? I think that no evidence can be furnished that additional deposits were laid down and afterwards removed. It is, as already mentioned, probable that the muck had been accumulating ever since the beginning of the Kansan glacial stage, and I believe that the fossils found testify to this proposition. If, now, this is true what becomes of deductions based by Dr. Chamberlin on the skillful work which he did at Vero?

Dr. Chamberlin<sup>12</sup> emphasizes the importance of the presence of the pottery found at Vero. No pottery was found in stratum No. 2. However, nobody has the knowledge or the authority to say that pottery was not used in America by Pleistocene man. As for myself, I would say that its presence in No. 3 is evidence that early man did use it. Recent revelations indicate that in America in Pleistocene times the art of working flint was far more advanced, in some tribes at least, than had been suspected. The same may be true as regards pottery.

It is the writer's conviction that Dr. Chamberlin erred as respects the age and origin of the coastal terraces, the age of the Anastasia marl, the ages of the creek beds and of the bog sandstone, the origin of the spherical concretions, the manner of accumulation of the bones, the composition and fate of the various elements of the fauna, and the position and age of the human remains. Nor can I give assent to any one of the four conditions set forth at the close of his second report.

The geologists appeared to be in agreement that there had passed between the deposition of stratum No. 2 and No. 3 no considerable lapse of time. In that case the apparent break may mark the begin-

<sup>12</sup> Journ. Geol. 25: 682. 1917.

ning of the Kansan glacial stage. The change of climate produced a more swampy condition of the little valley and made it a less agreeable resort for such of the larger animals as yet remained and there was a denser growth of plants. The muck accumulated slowly. There appears to be no evidence of either elevation or depression. If the time that has elapsed be taken as 300,000 years and the thickness as 50 inches the amount added would be one inch in six thousand years. The upper layers may be comparatively young; the lower, very old. While it is possible that some bones were washed up from the lower layer there is no necessity for granting it, for they belong to species which continued to live in that stage.

Dr. Aleš Hrdlička's theory of the presence of human bones in the deposits at Vero was short and simple. They were purposely buried there. No claim was made that there was any visible disturbance of the sand, marl, and muck such as would be caused by digging and refilling the grave. There might at first have been some unnatural mingling over the cadaver, but the materials would soon regain their former relations. He reported that evidences of this tendency to reestablish original conditions were observed already on the dump left by the steam excavator.

Dr. George Grant MacCurdy, of Yale University, recorded his conclusions in two papers.<sup>13</sup> In each article he figured three of the flint spalls collected by Dr. Sellards. Two were found in stratum No. 2. One of these was shaped somewhat like the blade of a broad ax. The height was one inch; the length of the thin edge was an inch and five-eighths. Dr. MacCurdy's explanation of its presence in the middle bed was that it had worked its way down by the aid of growing roots or burrowing animals. One may be curious to learn at what point of such a spall a root-cap could strike so as to guide it down through a bed of muck. More spalls were found in No. 2 than in the bed above it. Might not one as well assume that some had been washed up from the lower bed into the upper one? The number of animal burrows that have been dug in our broad land may be just a little short of infinite and arrow heads and spalls might work their way into these; but has any anthropologist ever found a flint weapon in such a situation? In the muck bed at Vero fragments of pottery were abundant. How has it happened that none of these were assisted to reach No. 2 either by roots or rodents? The specious value of such explanations was definitely exposed some years ago by the

<sup>13</sup> Journ. Geol. 25: 56-62. 1917; Am. Anthr. 19: 252-261. 1917.

investigations made at Trenton, New Jersey, by the American Museum of Natural History.

Dr. MacCurdy fell into the same error as Dr. R. T. Chamberlin and various other people, that of regarding the "fauna" found at Vero as an integral thing which existed for a while and later disappeared. I have shown already a number of genera which characterized the first interglacial stage as revealed at Vero and numerous localities and which appear at no later stage. Dr. MacCurdy mentions as occurring in the upper stratum (No. 3) at Vero *Elephas columbi*, *Mammot americanum*, *Chlamydotherium*, horse, and tapir. *Chlamydotherium* may have lived on for a while in the Kansan stage. It may have possessed some of the vitality of its near relative, the armadillo, which is still living in Texas. As for *Mammot americanum* and *Elephas columbi* and certain peccaries, they continued on probably all over the continent down close to or within the Recent. *Myiodon* and some species of tapirs and one of the horses found at Vero, *Equus complicatus*, and possibly *E. leidyi*, held on until after the Illinoian glacial stage.

After this article had been put in type the announcement was made by Dr. J. W. Gidley, of the U. S. National Museum, that he had found, in two or three localities in Florida, human bones and artifacts definitely included within stratum No. 2. These discoveries ought to end the dispute about the relationship of man to this important deposit.

PALEONTOLOGY.—*Characters of the brachiopod genus Lingulidiscina Whitfield.*<sup>1</sup> GEORGE H. GIRTY, U. S. Geological Survey (Communicated by JOHN B. REESIDE, JR.).

Many years ago, in the course of studying certain faunas from northwestern Arkansas, it became necessary for me to deal with a large series of discinoid shells, and, while discussing the identification of the species I ventured to glance at the generic name that should be used for them. These shells belonged to the group for which Hall and Clarke had revived D'Orbigny's term *Orbiculoidea*, but it appeared to me that on their own showing *Orbiculoidea* was a synonym of *Schizotreta*. Under these circumstances I cast about for some name that was already in the literature rather than propose a new one, and provisionally adopted *Lingulidiscina* Whitfield. Now Whitfield's

<sup>1</sup> Published by permission of the Director of the U. S. Geological Survey. Received March 3, 1928.

description of *Lingulidiscina*, if taken literally, would make that name inapplicable to the orbiculoideas of Hall and Clarke, but I had reasons for believing that the description was in certain respects not accurate. I was led to believe that *Lingulidiscina* could be used to replace *Orbiculoidea*, among other things, by the fact that Schuchert in his bibliography of American fossil Brachiopoda had included under *Lingulidiscina* a shell that I knew to be a characteristic *Orbiculoidea*, and as I had usually found Schuchert well informed and accurate, I concluded, without inquiry, that he must be in possession of some esoteric knowledge regarding those genera. My confidence in this instance now seems ill-judged in view of the singular compilation that passed as the genus *Lingulidiscina*. Thus we have (1) *Orbiculoidea newberryi* in which both valves are in agreement with *Orbiculoidea* as generally understood; (2) *Oehlertella pleurites*, in which the upper valve is like *Orbiculoidea* but the lower valve entirely different, the pedicle aperture being a notch in the margin instead of an oblique tubular perforation; and (3) *Lingulidiscina exilis* itself, in which the lower valve is like *Orbiculoidea*, but the upper valve different.

Some years after my comments on *Orbiculoidea*, in 1912 to be exact, Professor Prosser<sup>2</sup> took a hand in the *Orbiculoidea* question and quoted a letter from Professor Schuchert to the effect that *O. newberryi* was included under *Lingulidiscina* by mistake. This admission was perhaps unfortunate because otherwise Schuchert might lay claim to almost superhuman penetration in an allocation that, on the face of things far astray, now appears to be very close to the truth. Prosser not only made this allegation against my use of the name *Lingulidiscina* but seemed to think that Hall and Clarke were entirely justified in their use of the name *Orbiculoidea*. Though I could not agree with Prosser on this point, and though one of my reasons for substituting *Lingulidiscina* was shown to be fallacious, I continued to use *Lingulidiscina* until very recently, partly because I felt disinclined to reopen the discussion and partly because *Lingulidiscina* still seemed available on fairly good grounds.

Now Professor Schuchert's inclusion of *Orbiculoidea newberryi* under *Lingulidiscina* was not my only reason for thinking that *Lingulidiscina* could properly be employed for these Devonian and Carboniferous shells. Indeed, I found great difficulty in understanding how, as was said to be the case in *Lingulidiscina*, a brachial valve that had essentially the shape and general plan of construction of

<sup>2</sup> C. S. PROSSER. Bull. Ohio Geol. Surv. (4) 15: 203. 1912.

*Lingula* could be mated with a pedicle valve that had essentially the shape and general plan of *Orbiculoidea* (*Discina*) in view of the fact that these plans are so unlike that the two genera are actually assigned to different orders of brachiopods, *Lingula* to the Atremata and *Orbiculoidea* to the Neotremata. I felt that Whitfield's characterization could hardly be taken literally and that as his figures show the pedicle valve to have typical discinoid characters, the brachial valve was probably of the same type, though possibly having an apex uncommonly near the posterior margin. These considerations appear not to have occurred to either Prosser or Schuchert, and neither of them seemingly tried to ascertain what the characters of *Lingulidiscina* really were. The facts could be ascertained only through an examination of the type specimens, and these, through the unfailing courtesy of the American Museum of Natural History, I have been able to study. My observations in this field seem worth recording even though I now accept *Orbiculoidea* as a valid name in the sense adopted by Hall and Clarke, for they help to establish the relations of *Lingulidiscina* to other genera, relations which Whitfield's diagnosis left more or less doubtful. If his diagnosis were taken without qualifications, *Lingulidiscina* could hardly be of lower standing than the type of a new family. One might even go a little further and say that a brachiopod in which one valve had a terminal beak with shell accretions only at the front and sides while the other valve had a central beak with shell accretions equal all around, could not possibly occur in nature.

The generic description of *Lingulidiscina* reads thus:

"An inarticulate brachiopodous shell, in which the upper valve is linguloid in character, having a marginal or an essentially terminal beak, the accretions by growth being along the lateral and basal margins; lower valve discinoid in character and having its growth lines nearly equal on all sides of the initial point and perforated on the cardinal side by a byssal slit or opening, as in *Discina*. Shell structure as in *Lingula* and *Discina*. Muscular scars yet unknown. Type, *Lingula exilis*, Hall."<sup>3</sup>

The type species of *Lingulidiscina* is commonly quoted as *Lingula exilis* Hall. This is possibly in error. Hall figured two specimens of *L. exilis*, one of which was subsequently figured by Whitfield in illustration of the genus *Lingulidiscina*. Figure 8 of Hall was described as "a specimen with the beak imperfect"; figure 9 as "a more convex individual which may belong to the species." The language here employed clearly implies that Hall was in doubt about the specific

<sup>3</sup> R. P. WHITFIELD. Bull. Am. Mus. Nat. Hist. 3: 122. 1890.

identity of these two specimens and that the one shown by figure 8 should be considered as the type of *Lingula exilis*. On the other hand, it is the doubtful specimen that was later figured by Whitfield as belonging to *Lingulidiscina*. If Hall's two specimens are really conspecific with each other, and if the doubtful one is in turn really conspecific with the specimens that furnished the generic characters of *Lingulidiscina*, then *Lingula exilis* is in fact the type species of that genus. The doubtful specimen is among those loaned me by the American Museum of Natural History and I feel confident that it is an *Orbiculoidea*. The other and typical specimen of *Lingula exilis*, I have not seen, but the growth lines in Hall's figure suggest that it is really what he believed it to be—a large *Lingula*. If such is the case, *L. exilis* obviously is not the type species of *Lingulidiscina*. I have not thought it necessary to borrow the type specimen of *Lingula exilis* in order to form an opinion upon this point, for my inquiry is addressed at present more particularly to ascertaining the characters of *Lingulidiscina*, and these depend upon Whitfield's specimens and not on Hall's.

To sum up my conclusions regarding *Lingulidiscina* before commenting on the type material in detail: Whitfield's specimens are poorly preserved, probably exfoliated, certainly somewhat crushed, and certainly more or less broken at the margin. Both valves are constructed essentially as in *Orbiculoidea* of Hall and Clarke, though the apex of the upper valve is more excentric than is common in that genus. Whitfield's description is misleading, if taken literally, in saying that the valve is "linguloid in character, having a marginal or an essentially terminal beak, the accretions by growth being along the lateral and basal margins." Let us consider the type specimens in detail, first those representing the brachial valve:

Figures 1, 2, and 3 in Whitfield's description of the genus represent the same specimen, figures 1 and 2 being different views of the brachial valve. These figures, which show a shell shaped like *Lingula* with a beak apparently terminal at the pointed posterior end, are accurate enough in so far as they represent the specimen as it now is, but they are highly misleading in so far as the specimen is decidedly imperfect. The growth lines run out half way up the sides of the brachial valve indicating that the shell was broken in the marginal parts and that the posterior outline was originally much less pointed or at least that the present outline is far from being the true outline. The point represented in Whitfield's figure as a terminal beak appears to be the true apex of the valve, so that the apex must originally have been situated some distance from the margin.



Where discinoids were buried with both valves in conjunction they often suffered a lateral displacement due to compression, one valve projecting on one side, the other on the other. When such specimens are broken from the rock the fracture is likely to occur where the valves are in contact, causing them to be defective along opposite sides. This condition is apparently exemplified by the specimen shown by figures 1, 2, and 3, the brachial valve having slipped backward with the result that the brachial valve projected beyond the line of contact at the posterior end and the pedicle valve projected a corresponding distance at the anterior end. The evidence for this interpretation is as follows: The brachial valve, even in its broken condition extends considerably beyond what appears to be the true posterior margin of the pedicle valve. In addition to the specimen illustrated, however, we have the slab from which it was detached. This slab retains the impression of the pedicle valve and also, projecting downwards from it almost at right angles, a considerable strip of shell, which appears to be the marginal part of the overlapping brachial valve. This strip of shell, which surrounds the pedicle valve from part way up the left side to part way across the posterior end, where it is broken off, may, it is true, be a section of the pedicle valve itself, folded over at a sharp angle, but from the apparently small amount and general direction of the compression suffered this explanation is not so likely. In any event, the brachial valve is undoubtedly imperfect around the posterior margin and the idea conveyed by Whitfield's figures is highly misleading. They represent the specimen as it is, without showing that it is fragmentary, and they seem to bear out the generic diagnosis in a way that is most deceptive. They show, it is true, a shell that at first recalls some of the broader, more spatulate lingulas, but in *Lingula* it is the pedicle valve that projects and is pointed, and the brachial valve that is short and more blunt at the posterior end; here the relation is precisely reversed, the brachial valve is long and pointed, the pedicle valve short and rounded at the posterior margin. This is, to be sure, not at variance with what the description says, though it is at variance with what the description seems to imply—an agreement of some vital sort with *Lingula*. However this may be, the relation between the valves as they now exist in the specimen are in all essentials as they are shown in Whitfield's figure 3, the pedicle valve rounded across the posterior end, the brachial valve pointed and projecting well beyond it. Such, one can say with almost perfect safety, could not possibly be the original condition of any brachiopod shell and the fact affords clear evidence, if